DIFFERENTIATION IN INTERCRUSTAL MAGMA BASINS

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Dr. N. L. Bowen's comprehensive article on "The Later Stages of the Evolution of the Igneous Rocks," issued as a supplement to the last volume of this *Journal*, will be hailed with satisfaction by all petrologists, and indeed with gratitude by those who have the misfortune not to be chemists. It contains the first serious attempt to deal with the problem of magmatic differentiation directly from the standpoint of experimental knowledge. In demonstrating how the course of crystallization may be changed by the sinking of crystals, or the straining away of liquid from crystals, or the formation of zoned crystals in isomorphous groups of minerals, the author scarcely goes beyond actual laboratory experience, and his conclusions accordingly carry a great weight of authority. When he proceeds to construct on this basis a general theory of differentiation, the element of hypothesis is necessarily introduced, and, as the author recognizes, his argument can no longer command unquestioning acceptance. It is a very interesting contribution to a discussion which is not likely soon to be closed.

I wish to make a few remarks upon one of the subsidiary issues, which, however, touches the main theory at numerous points, viz., Bowen's predilection for differentiation *in situ* as opposed to differentiation prior to intrusion. That an appreciable settling down of crystals may take place after intrusion is not to be denied, but I think that the experience of any field geologist goes to show that it is a rare and exceptional incident. Daly has given a list of about thirty stratified sills and laccolites in which such "gravitative" differentiation is believed or conjectured to have occurred, but probably a critical examination would dispose of many of the examples cited. In some, such as the Loch Bordan mass in Sutherland, there is not a gradual transition but a sharp boundary between the several

rock types. Bowen remarks that the uppe acid magma, remaining fluid after the lower basic portion has wholly crystallized, may come to have an intrusive relation to the latter. This would be sufficient to explain veining of the one rock by the other; but, where an overlying sheet is separated from an underlying one by a surface of discontinuity, I can see no explanation but that of distinct intrusions. Nor is this explanation necessarily excluded even when no sharp division is seen, for, under appropriate conditions, a transitional zone may result from partial admixture. The Sudbury laccolite is probably a case in point, though I must confess to only a limited personal examination of the mass. I found no indication of a regular "composition gradient" in either norite or granophyre, considered separately, while the transitional zone between them has all the characters of a hybrid rock. The sulphide ore I leave out of count, as doubtless representing a magma immiscible with that of the norite. The clear instances of gravitative differentiation in sills and laccolites of which I have direct knowledge are all in rocks which must represent very unusually fluid magmas, such as the analcime-bearing intrusions of Permian age in Scotland. They are the kind of exceptions which help to prove the rule: viz., that in an intrusive body of moderate size a prohibitive viscosity soon puts a stop to the settling down of crystals. Doubtless a laccolitic mass of very large dimensions retains its fluidity longer, but it is obviously in a great intercrustal reservoir that the most favorable conditions for this action will be realized.

Bowen would apply the conception of differentiation in place to the plutonic rocks of Skye; but the facts, as I see them, absolutely negative such a hypothesis. The peridotite is not found at the base of the gabbro, but enveloped in the midst of it. The granite breaks through the gabbro, and, where any approach to a stratiform arrangement is apparent, does not overlie, but underlies, the basic rock. In one part the granite has been so chilled against the gabbro that its margin and the offshoots from it assume the characters of a spherulitic rhyolite. I infer that the gabbro was, in this place, not only solid but cold when the granite was intruded. The large gabbro laccolite itself is made up of numerous irregular sheets, showing differences in composition and structure, and often

visibly cutting one another. In the peridotite this composite structure is more strikingly exhibited, and it can be detected in places in the granite, which is a much more uniform rock. The several component sheets are not disposed in an orderly fashion in accordance with their various densities. Add to this evidence the fact that peridotite, gabbro, and granite all make smaller separate intrusions, some much too far away from the main complex to have any direct connection with it, and it will appear beyond dispute that the differentiation which yielded these various rocks was effected prior to their intrusion, and therefore in some large reservoir at a deeper level.

Bowen does not refuse the conception of a deep-seated magma basin stratified according to density; but he seems to think it an absurdity that, on that hypothesis, the earlier intruded magmas should be drawn from the lower levels (p. 73). I will try to remove his objections. I have already urged[1] that in order that such a basin may have a considerable degree of permanence, as it obviously has, we must suppose some approach to thermal equilibrium between it and the surrounding crust. This implies a temperature gradient within the basin approximately like the normal gradient in the earth's crust of the region. It implies, further, what I may call a fusibility gradient corresponding with this normal temperature gradient. Now, the separation and sinking of crystals, as pictured by Bowen, goes with a cooling-down of the magma, which terminates in complete solidification. Any intrusions drawn from the basin must therefore be consequent upon *remelting*. The occasion of this I presume to be a gradual rise of the isothermal surfaces, which must accordingly become more closely spaced. In other words, reheating implies a temperature gradient steeper than that to which the fusibility gradient is adjusted, and it follows that the lowest layers will first become fluid. I have not attempted to develop this view of the matter, and should welcome criticism; but Bowen's zeal for differentiation in place has caused him to pay little regard to the possibilities of this alternative.

I am wholly in accord with Bowen in the conviction that alkaline and calcic rocks are derived from the same primitive magmas

[1] See especially *Compte Rendu XII Congr. Géol. Intern.*, Toronto, 1914, pp. 205–8.

(p. 59). My belief has been, and is, that the differentiation of these two great classes of magmas from the common stock and the separation of them—in general in a horizontal sense—constitute the first and most important steps in the evolution of igneous rocks. Why the chemical differentiation should so consistently follow these lines has been a difficult problem, and it is the more gratifying to be offered at least a partial answer to the question. Stated broadly, Bowen's ideal scheme of differentiation leads first to a series of calcic rock types and subsequently, if continued, to an alkaline series. There are qualifications of this rough statement which I do not go into here; but in general it appears that, if a separation can be brought about at a certain well-defined stage of the progressive differentiation, it will be a separation between calcic and alkaline. This separation, I hold, has actually been effected on a grand scale, and I have sought the immediate cause of it in the action of crustal stresses squeezing out the residual fluid.

A discussion of this suggestion from the chemical point of view would be instructive, but here Bowen disappoints expectation. He dwells on particular cases in which separation has not taken place at the stage specified, but at a somewhat earlier stage; and he throws doubt upon the existence of any general regional distribution of alkaline and calcic rocks, such as Iddings demonstrated long ago. The fact that, among the younger rocks of North America, alkaline types characterize the Atlantic slope and calcic the Pacific, he would explain by supposing that erosion has exposed deeper levels on the western side of the Rocky Mountains than on the eastern. He forgets that the contrast of petrographical facies holds good for the lavas as well as for the intrusive rocks. Moreover, the fact that lava flows still cover vast areas on the western side, while on the eastern they have mostly been removed, makes it difficult to accept his statement about the relative amounts of erosion.

As regards the association of calcic rocks with regions subjected to powerful lateral thrust, nothing would be gained by traversing old ground again, but to Bowen or any other unbeliever I will offer just one consideration. If we examine those crystalline schists which are admittedly of igneous origin, together with foliated

igneous gneisses, we find that they belong almost exclusively to the calcic branch. A few exceptions there are, and must be. A nepheline syenite may be intruded in a line of faulting during the time of movement, as in the Langesundsfjord; or it may be crushed and metamorphosed long afterward by stresses with which it has no genetic connection, as at Loch Borolan; but these are isolated and incidental occurrences. The matter is easily brought to the test. In Grubenmann's classification, based solely on chemical composition, the crystalline schists and gneisses of igneous origin are contained in six of the twelve groups. The calcic rocks are in Groups I, III, IV, and V, which correspond with granites, diorites, gabbros, and peridotites. They include a rich variety of types, and collectively make up enormous tracts of the earth's crust. To complete his classificatory scheme the author has been able to produce various types of alkaline rocks, which scantily furnish forth Groups VI and VII, but most of them are little more than petrographical curiosities. In respect of the total bulk of all known occurrences, these alkaline crystalline schists as a whole are quite insignificant as compared with any single type in the calcic division.

The striking disparity here noted is only one consideration among others which points to a peculiar distribution of alkaline and calcic igneous rocks in relation to crustal stresses. If anyone seriously believes that such things are matters of chance coincidence, there is no more to be said. It is to be hoped rather that chemists, as well as geologists, will recognize here a real significance, and will lend their help in the attempt to explain the facts, not to explain them away.